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OF THE CHILDREN'S HOSPITAL

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(1) Diamond, L. K.; Smith, N. J., and Vaughan, V. C., III, in Nelson, W. E.: Textbook of Pediatrics, ed. 6, Philadelphia, W. B. Saunders Company, 1954, p. 962. (2) Nitzum, W. L.; Jackson, R. L., and Stearns, G.: A.M.A. Am. J. Dis. Child. 86:553, 1953.

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POSTMATURITY WITH PLACENTAL DYSFUNCTION

Stewart H. Clifford, M.D.*

It is an honor for me to be selected to give the Joseph Wall Memorial Lecture. When I was informed by Dr. Washington that I had been invited to give this lecture, I told him that nothing could give me more pleasure. In a way, it would be an opportunity to pay a long standing debt that all of us, the country over, owe to Dr. Wall. The great of American pediatrics are fast fading away, and very soon all of the pioneers will have left our midst. Joseph Wall was such a man. His contributions are invaluable, and his memory always creates a warm glow in our hearts. I can remember Dr. Wall at Academy meetings when he was busy effecting the reorganization of the Academy, and accomplishing the changes in the Constitution and By-laws with his infinite tact and fine sense of humor.

The subject of today's talk is one that is surrounded by a great deal of confusion. For this reason my title and the original title under which we published was not "postmaturity" per se, but "postmaturity with placental dysfunction". We would like to have published under the term, "placental dysfunction" alone, but from the point of view of reaching the group in which we were interested, and from the point of view of having the material we wished to submit catalogued in the *Indices Medicus* in the proper place, it was necessary to tag this condition "postmaturity".

BACKGROUND

To most obstetricians, at the time we began publishing, the mention of the word "postmaturity" was like waving a red flag at a bull. Many textbooks covered the subject of postmaturity as if it were a "figment of the imagination"; "there was no such animal". This was the status of postmaturity in this country up until a very few years ago. Strangely enough, while American professional people were ignoring postmaturity, there was a great deal written in the medical literature referring to this particular subject on the Continent, in England, and in other parts of the world. It has been only recently that the obstetrical fraternity has really seriously begun to consider this problem in its broad aspects. My feeling is that much of the confusion that surrounds this subject is analogous to a similar situation which surrounded the subject of prematurity at the time when I began

From a transcription of the Second Annual Joseph Wall Memorial Lecture given on May 26, 1956 as part of the Children's Hospital of D. C. Alumni Association Meeting.

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to study medicine. At that time prematurity per se was accepted as a cause of death. Now, as we know, except for the extremely small and immature prematures, this is no longer the case. We have to know what is wrong with the premature, what causes morbidity and mortality. With this change in philosophy, we began to make progress in the study of the premature problem. We think of prematurity as a time in life when patients may have problems peculiar to that chronologic period. In the same way, I think that we must consider postmaturity as also referring to a chronological period during which we collect a group of patients that not only has the difficulties that others have, but has some problems peculiar unto itself.

DEFINITION

To discuss postmaturity, there are several reasons why we must have a definition. There is a legal reason; in fact, the first interest in postmaturity arose out of social and legal problems, the question of legitimacy, for instance. Then, from the clinical point of view, we have to have a definition in order to select a group of infants who require a special type of care. Also, we need a definition in order to allow comparison of groups of patients from one clinic to another, so that we can analyze and evaluate various methods of management. In the past, one of the roadblocks was that postmaturity was held to be synonymous with the "giant" fetus. Let us dispel that once and for all as it has already been dispelled in the obstetrical literature. There is no connection between postmaturity and the so-called "giant" fetus, usually defined as one over 9 pounds, 13 ounces. At the turn of the century it was believed that a patient who was past term was getting a bigger and bigger baby. Therefore, it was reasoned that delivery was going to be difficult due to the presence of this giant fetus, and an early termination of labor was recommended. This was in fact put into practice, only to find that more harm than good was done. These "postmature" mothers did not go into labor satisfactorily and when they did they had very difficult labors. Uterine inertia was commonplace, and the fetal and maternal mortality so great that the whole policy of interference came into deserved disrepute.

Now we know that the rate of growth of the fetus proceeds quite regularly and rapidly to about the 260th day of pregnancy, then continues to increase but not at the same rapid rate for the next 20 days until 280 days, after which there is practically no change in the growth of the fetus. If anything, there is a tendency to lose weight. In other words, a baby who is going to be a big baby, a giant baby, is one that has reached that size a week before term, and any further prolongation of pregnancy has very little effect on the total weight of the infant.

There are still many people, however, who feel that although the post-

mature baby does not get to be a huge baby, he still has to reach a certain minimum weight before he can be considered to be postmature at birth. Some, in fact, have set a figure as high as 4,000 grams. A baby below 4,000 grams in this classification could not, under any circumstance, be considered a postmature baby. Other minimum limits of weight that have been suggested vary from 3,250 grams to 4,500 grams. Nesbitt, in his most recent and excellent review of the subject demands that a baby be at least 4,500 grams, or 9 pounds 13 ounces, before he is to be considered a postmature baby. However, there are other authors who take the opposite point of view, that a postmature baby has to show evidence of loss of weight. Finally, there are many workers who find no relation at all between birth weight and postmaturity.

In the group of patients we have analyzed we have found, as you will see a little later on, that postmature babies can be above average weight. They seem to increase in weight *in utero*, after the 294th day, but at a relatively slow rate. There is no consistency about it, and the hypothesis that we hope to develop this afternoon tends to explain some of the discrepancy that has crept into the literature about these particular babies. Our philosophy is that a postmature baby with a normal placenta will go on growing and increasing slightly in weight as long as the placenta is able to furnish adequate nourishment. A senile placenta, one that fails to provide the proper nutrition, may throw the baby on his own resources. Under these conditions the postmature baby may actually lose weight *in utero* and be considerably below the expected weight for his gestational age and for his actual body length. We believe that this is the reason for the discrepancy in the literature: that the ultimate size of the baby depends on the physiological function, degree of maturity, and absence of degeneration in the placenta itself.

Another method of gauging the size of a baby is by length. Here again the results are as confusing as are those for weight. It is impossible to define a postmature baby on the basis of any given length below which he is not postmature and above which he is.

Finally we come to the question of gestational age. This has been the traditional criterion, and we all agree that it is a very unsatisfactory crutch to lean on before the baby is born. In reviewing the work of some 30 authors surveying a total of over 200,000 deliveries in which gestational age is used as a criterion, we found that 25 per cent of all pregnancies go beyond the 287th day or a week after term. Twelve per cent go beyond 294 days, or two weeks past term, and 5 per cent go beyond 301 days, or three weeks after the expected date of confinement. It is extremely interesting to find how uniform the incidence turns out to be in the various series. I believe that the figures just given represent a true cross-section of the obstetric population and that these are the proportions of gestational ages found in

the normal consecutive number of deliveries that one would encounter. It is needless to go into the arguments against gestational age as a criterion. While the gestational age seems to be accurate in the vast majority of cases the patient you have under current consideration is the one you are interested in. And there you will always have doubt, as we all do, whether the dates are correct. However, from the point of view of definition, we agree pretty much with Dr. Eastman's contention that a true postmature baby by gestational age would be one that was three weeks or more beyond term. Others will use two weeks or more beyond term. It makes little difference what classification you use as long as you define it and present your results according to that definition.

PRESUMPTIVE SIGNS

In addition to gestational age there are other presumptive signs of postmaturity which are extremely valuable in diagnosis. These signs are of great importance because they may be present at a time when it is necessary to make a decision. We cannot wait until the baby is born with the signs and symptoms which we think are characteristic of postmaturity. We cannot wait for the baby to die *in utero*. The problem as it usually presents itself to the obstetrician is of a pregnant woman who is two weeks overdue; does this mean something or does it not, and should he do some thing or should he not? He has to make up his mind and plan a program of action or inaction. He needs additional help from these presumptive signs.

One sign which is quite well recognized in certain instances of postmaturity is a decrease in amniotic fluid production resulting in oligohydramnios. You may actually see the uterus shrink in size during the period of observation. The amniotic fluid apparently becomes absorbed or resorbed. We have had a number of cases where this resorption has taken place. Shrinkage of the uterus measured either by reduction in distance from the symphysis pubis to the uterine fundus or by reduction in the circumference of the abdomen, is a very significant sign and has been quite reliable in pointing to possible difficulty.

Walker has discovered what may also be quite a useful tool in diagnosis. He found that many mothers who delivered postmature infants, as they went beyond term, began to lose 2, 4, or 6 pounds a week instead of continuing to gain or remaining at the same weight. He has found that this is very frequently associated with a shrinkage in uterine size and diminution in size of the fetus, and has felt that here is another bit of presumptive evidence which we can add to our clinical records.

Fetal distress may be another presumptive sign of a postmature pregnancy about which we are concerned, a postmature pregnancy accompanied by a diseased placenta. Here some very interesting work has come forward.

A recent article from Capetown reports that when signs of fetal distress (primarily alteration of the fetal heart rate) accompany the passing of meconium in fetuses who are postmature, the fetal death rate is extremely high, approaching 80 per cent. When postmaturity is unaccompanied by either of these two phenomena the mortality rate does not differ from standard mortality rates.

How are we going to tell clinically before the fetus is delivered, whether or not he has passed meconium? Walker has solved this problem to his satisfaction by inserting a long, curved catheter into the cervix and passing it up to the fundus of the uterus. He then plunges it into the hind waters at the fundus and takes out a sample of the amniotic fluid. If the fluid is clear, he knows that this baby has not been suffering from anoxia, in any event not to the extent of causing the liberation of meconium, and he sits tight. If, on the other hand, he finds that the fluid is meconium-stained he believes that the baby has already been subjected to severe anoxia. He feels that adding the further anoxia and unavoidable trauma of vaginal delivery to the tissues of a baby who is already anoxic prior to labor will increase mortality and morbidity tremendously. His philosophy is, under those conditions, to deliver these women immediately by caesarian section, and he believes that he has significantly reduced the neonatal and fetal mortality rate by this means.

I have recently heard of other work currently being performed in New York which may also be of great help to us. A group of workers, I believe at Mt. Sinai, is studying the electrocardiogram of the fetus *in utero*, and has found that in postmature infants with damaged placentas, the fetal heart already exhibits a prolongation of conduction time and other changes characteristic of a heart suffering from anoxia. We are gradually collecting various types of presumptive evidence which will help the obstetrician to separate the relatively few babies who are in need of immediate action from the babies who are just postmature and need no particular care at all.

ETIOLOGY

We now come to the question of why these infants become postmature in the first place. There are general theories. One is the hormonal theory. There are those who feel that preceding normal labor there must be a reduction in progesterone which conditions the uterus to respond to an induced labor. Rosencrantz and Ramirez studied the placentas of postmature infants and found that instead of having a diminished progesterone content the placentas contained two to six times the normal level of progesterone and about half the normal level of estrogen. They believe that the normal progesterone level, for one reason or another, fails to diminish and pregnancy goes on. We do know that Franklin Snyder with his work in

rabbits was able to prolong a pregnancy, prevent the onset of normal labor, and produce postmaturity by the injection of this type of hormone.

Then there is the question of hereditary and constitutional factors. It is well known that many women who have postmature babies have them in series; baby after baby is postmature. There are investigators who feel that there is a very high incidence of congenital anomalies in this group, and they postulate that the reason these babies become postmature is due to the presence of hydrocephalus or anencephalus. In other words, in order for labor to be initiated, the head or the presenting part must descend into the lower uterine segment, and set up the reflex of Ferguson which initiates the cycle of labor. A great many people are inclined to think that malposition, cephalopelvic disproportion, or anything that keeps the fetal head above the pelvic brim, is responsible for causing a pregnancy to continue beyond the expected time of delivery.

Another group of investigators believes that genital infantilism in the mother is responsible. I am very much impressed with the number of women delivering postmature infants who do fit into some type of infantilism syndrome. These women are very apt to have a male-type pelvis; they are very apt to have a late menarche; they are very apt to have had a history of irregular menstruation; a great many of them have been previously investigated for sterility; and many of them have their first child late in life. In the patients whom I have seen in consultation with obstetricians in our clinic, there is a rato of at least three patients to one referred from my friends who are working with sterility problems. I believe, in addition to other factors, that the genetic type of woman who has relative sterility, has her first baby late in life, and has a male type pelvis, is the one who is very apt to get into trouble with the postmaturity placental dysfunction syndrome.

Normally, the ratio of the weight of the fetus to the placenta is about six to one. I became interested in the subject and arranged all the fetal deaths at the Lying-In Hospital over a 12 year period according to gestational age of death and found that this six to one ratio remains the same for all gestational ages except in erythroblastotic babies, who as you well appreciate, have very large placentas. Irrespective of the degree of maturity or the size of the infant, this ratio is maintained at a very stable level. I am wondering if, in postmature infants, the placenta has only a given size which it can reach. It functions in proportion to this size and no matter how long the pregnancy lasts the placenta is not able to service a bigger baby than is dictated by the placental fetal ratio of one to six. If a given placenta is potentially capable of increasing in weight to only 500 grams and the baby is not delivered at term, it may be true that after he reaches the point of maximum growth for that size placenta, this baby with his continuing

demands will develop an oxygen and nutritional lack. We have all seen babies like this; I saw one again just the other day. It was a *bona-fide* full-term pregnancy but the baby weighed only 4 pounds 5 ounces and the placenta was a little nubbin about the size that one would associate with an ordinary premature baby of this size.

One of the most important things to understand is that in postmaturity most of the mortality is encountered before the baby is born; it is fetal mortality. This is the phase of the postmature problem that is intriguing obstetricians tremendously at the present time, and I think it is agreed by almost everyone that with each day after 280 days of gestation, the fetus *in utero* is subject to an increasing hazard. Studies from all over the world have been quite uniform in describing this finding. For instance, in analyzing the fetal mortality in a total of 200,000 cases reported by 22 different authors, I found that fetal mortality in the age group of 280 days to 293 days was 42 per thousand. The fetal mortality for the group of 294 to 300 days was 60 per thousand, and the fetal mortality in the group older than 301 days, or three weeks past term, was 82 per thousand. We, ourselves, investigated 5,000 consecutive cases which we classified according to gestational age, and found results quite consistent with those I have recorded from the literature as a whole.

These figures take on added significance when we investigate the primiparous woman. In our series we found that in this primiparous group, one out of ten women whose pregnancy went beyond 300 days, would lose her baby and 85 per cent of those babies would be stillborn. If we go still further, the statistical point at which the mortality rate went up was around 27 years of age, so that in primiparous women, three weeks or more beyond term, and 27 years of age or older, there will be a fetal loss of one-third. So instead of extending yourself over the entire 5 per cent of deliveries which are three weeks or more beyond term, watch closely the groups in which most of the fetal morbidity and mortality occur, the primiparous woman, and primarily the elderly primiparous women.

CLINICAL FEATURES

What do postmature babies look like when they are born? We agree completely with Dr. Schick that the vast majority of these babies are probably benefited by postmaturity. I know that Dr. Schick feels very strongly about this; he likes to call these "supermature" babies, and he believes that their increased hibernation and "life of Riley" has improved them both mentally and physically. At birth they look three weeks old, they are wide awake, alert, and although they may have cracking of their skin and elephant hide, they are very sharp customers. Many, in other words, are perfectly normal infants, and have not been damaged a bit by postmaturity.

I think that we should constantly emphasize to the mothers who go overdue that the chances are that this is a wonderful thing, and that there is no harm in it. From the point of view of the obstetric and pediatric team, when the mother does become overdue, it should be realized that the pregnancy is entering a phase which requires special consideration.

When these infants do get into trouble, the baby falling into what we call Stage 1 may be observed to have pronounced peeling of the skin and rather marked weight loss. The skin feels like an overcoat which is a good many sizes too big. In Stage 2, when the placenta is undergoing some oxygen lack, the baby is stained with meconium. He is a very sad looking character, very malnourished, and has marked peeling of the skin everywhere. As this process goes on we enter Stage 3. It takes about 3 or 4 days before a baby bathed in meconium will have beginning staining of the cord and vernix, and it takes a week to 10 days before the vernix and the cord color change to the bright yellow color we see in Stage 3.

A few words about liveborn mortality: Stage 1 has no mortality, and the morbidity is comparable to what you would expect in any anoxic infant. Stage 2 has the highest mortality. The reason for the somewhat lower mortality rate in Stage 3 is, of course, that the more severely affected infants are stillborn. The babies in Stage 3 are the ones who survived the period of severe anoxia and were born a week or so later after this insult *in utero*.

In conclusion, I think we can summarize our present opinions regarding this problem from the pediatric and obstetric point of view very quickly. Postmaturity is associated with an increased fetal mortality. The fetal mortality is due to a diseased placenta. In those infants who die prior to the onset of labor, we believe from our studies that in half of them there has been no factor other than postmaturity itself as the cause of death. In those who have gone through labor and died, we found a combination of factors. In only 12 per cent of these was there no other factor but postmaturity present. In over 88 per cent of them there was some abnormal factor of labor: prolonged labor, inertia, malposition or very frequently, persistent occiput posterior. So it seems that in the vast majority of pregnant women who do go past term, the mechanical factor is a very important one in prolonging pregnancy independently of whether the mother is primiparous or multiparous.

It, therefore, becomes very important to investigate any woman with a prolonged period of gestation. Then is the time to re-appraise the entire clinical course of her pregnancy, and her present condition, to rule out if possible, some mechanical problem retarding the onset of labor. If she has a perfectly normal placenta, as most of these mothers have, she can tolerate this two or three weeks delay in delivery and deliver a perfectly normal baby. If (1) she is postmature and (2) on top of that her placenta is begin-

ning to decrease its functional capacity, there is a greater likelihood of increased fetal mortality, increased neonatal mortality, and of more importance, increased neonatal morbidity. These women constitute only a small group, but prevention of mortality and morbidity in this group is a challenge to the whole obstetric and pediatric team.

PROBLEMS OF THE NEWBORN AND PREMATURE INFANT*

Panel Discussion

William F. Burdick, M.D., Moderator,† Harry H. Gordon, M.D., Robert Barter, M.D., Leroy Hoeck, M.D., Stewart H. Clifford, M.D.‡

Dr. Burdick:

Each of our guests today will give a short talk; then any questions from the floor can be discussed by the panel. The panel members are: Dr. Harry Gordon, Pediatrician-in-chief of Sinai Hospital in Baltimore and Associate Professor of Pediatrics at Johns Hopkins University Medical School; Dr. Robert Barter, Associate Professor of Obstetrics at George Washington University; and Dr. Leroy Hoeck, Chief of Pediatrics at the District of Columbia General Hospital. I will call on Dr. Gordon first, to make such remarks as he cares to make concerning either Dr. Clifford's paper or the subject of diseases in the newborn in general.

Dr. Gordon:

My first comment is really to thank you, Dr. Burdick and Dr. Washington, for inviting me here and giving me a chance to hear this very well balanced presentation. I am periodically involved, Dr. Clifford, in the kind of arguments with obstetricians which I imagine you have had, and I am very happy to see the data that were presented today.

Dr. Clifford told us that we have to think of post-maturity just as we do of prematurity, as a time of life, and just as some infants who are born prematurely have problems, so some infants who are born post-maturely also have problems.

The comments which I wish to make are not about postmaturity but rather about some of the problems in the care of newborn and premature

* Presented as part of the proceedings of the Second Annual Children's Hospital Alumni Day on May 26, 1956.

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babies. When I first see a newborn baby who is not doing well, one of my chief problems is to find out what has happened before, from reading the nurses' notes. Because of the increasing birth rate and the shortage of nurses, we have simplified the nursing records of newborn babies as much as possible. This allots a minimum of time to charting and a maximum to direct care for the babies. Nobody will argue this, but when we have either a sick infant or an infant suspected of illness, neglect of charting increases the problem. We have to see to it that certain symptoms are to be considered as a signal for the nurse to start keeping the accurate notes that are kept for premature infants, or other sick infants in our wards. I do not propose to expand on this anymore. It is what has been called a problem in "hospital logistics" and I think it is a critical one.

There are three problems in care of premature babies on which I would like to comment briefly. The first one has to do with the use of oxygen. We all recognize that the administration of excess oxygen was responsible for retrolental fibroplasia and recommendations are now made that perhaps no premature baby ought to be put in oxygen unless he needs it. With the shortage of nursing time and with our inability to judge accurately the need for oxygen, we are not prepared to subscribe to this, and I am very anxious to hear how Dr. Clifford and Dr. Hoeck feel about this problem. Instead, we recommend that all premature babies under 1500 grams (three pounds and five ounces) be put in oxygen not to exceed 30 to 35 per cent concentration, and this to be measured every four, or at the most, every eight hours. If at the end of 24 hours, the infant is apparently doing well, then we take him out of oxygen. With this program we have seen virtually no retrolental fibroplasia; we hesitate to say none at all because the senior ophthalmologists are not so interested now in examining babies' eyes because there is no longer enough pathology. We have not seen any serious degree of retrolental fibroplasia, and as long as most babies who die do so within the first 24 hours, we have not been prepared to discontinue the routine use of oxygen for the small baby during the first day. It may be that data will some day be collected from a carefully controlled study which will indicate that this is not necessary. Such data are not now available, in spite of reports which say that the mortality of premature infants who are given low oxygen is not greater than the mortality of premature infants who are given high oxygen. This conclusion was drawn from the cooperative study, but this study group excluded all the 48-hour deaths, and therefore I say that there are no data which at this point will tell us that it is safe not to give oxygen during the first 24 hours. I will admit there are no data that say it is necessary; we still prefer to give it because we believe the risk of retrolental fibroplasia is very small.

The second item which I want to talk about has to do with hyperbilirubinemia in premature infants. You have read reports recently which indi-

cate that in the premature baby, the serum bilirubin may go up to a level higher than that seen in the full-term infant, and that furthermore kernicterus may be seen in premature babies even though the bilirubin does not rise above the "holy" figure, 20 milligrams per 100 ml. At any rate, the recommendation is now being made by some that, if the serum bilirubin in a premature baby gets above 15 milligrams per 100 ml., an exchange transfusion should be given. There is not at this point any evidence to indicate that this is necessary. We all accept that some premature babies do develop kernicterus with no known cause for their increase in bilirubin. It seems to me, however, that exchange transfusion, on the basis of present data, represents a radical step. We have seen some infants in whom jaundice and sluggishness developed on the fifth and sixth day, particularly if we were very slow about getting fluids into them. In some of these infants the administration of five per cent glucose by stomach tube in large amounts or the use of small clyses, has produced a striking change in the baby's clinical condition.

The third item, about which there has been some argument recently, has to do with the feeding of premature babies. People ask us whether we are still committed to the use of half-skimmed cow's milk with carbohydrate. The answer is yes, and we fall back on data collected at the New York Hospital 15 years ago. We know of no data to contradict this. These data state very specifically that if one compares the rate of weight gain on half-skim milk formula with that of breast milk or simple evaporated milk formulas, there is no difference in weight gain for larger premature babies. In the case of the small babies born with low stores of protein, and with defects in fat absorption, it seems that high-protein, low-fat mixtures produced an increased weight gain. As to the recent talk that these formulas are high in ash and that this high ash hurts the babies, there are no data in the literature to support this. As a matter of fact, one could almost question whether a little bit of extra ash might not help these babies gain. They do have to put more calcium and phosphorus into their bones; they need more phosphorus because of their rapid weight gain; and it is possible that some of the data to which I refer indicate that ash did the babies some good. What we come down to, in fact, is everything in moderation, and that includes the feeding of premature babies.

Dr. Barter:

I think that Dr. Clifford was very kind to the obstetricians in general in his talk on post-maturity. Perhaps he was too kind, because there are many obstetricians who, unfortunately, do not think there is such a thing as post-maturity. They still favor the time honored cliche of "when the apple is ripe, it will drop from the tree". If the Florida orange growers waited until all the oranges dropped from the trees, there would not be many

oranges sent North, and I think that a similar comparison is perhaps in order; with the changing scientific picture, there is no longer any reason to think that every baby is born at exactly the right time because it happens to be a product of nature. We now know that that is not so.

The work of Dr. Walker of Scotland is not as yet universally accepted in this country, but our group thinks that it has a great deal of genuine merit. As Dr. Clifford mentioned, when Dr. Walker ruptures the membranes of a patient who is postmature by date and finds that the amniotic fluid is golden yellow in color, he uses that as his criterion that the baby is postmature and in some degree of oxygen want. He has shown that after 43 or 44 weeks of gestation, in a third of all the stillborns that he had, there was no apparent cause for the fetal death.

On the other hand, if one ruptures the membranes in a patient who is postmature and the amniotic fluid is unstained or perfectly clear, then I do not think that one has to be overly concerned about that particular baby. It is also my feeling that the presence of meconium in the amniotic fluid is of much more significance than an irregularity in the fetal heart rate.

There is one thing about which I cannot quite agree with Dr. Clifford. I have not seen any significant degree of post-maturity in patients who have cephalopelvic disproportion. In our experience with x-ray pelvimetry, I have not been impressed with the fact that patients who have small pelves do not go into labor at the appointed time. I think that in the absence of x-ray pelvimetry, many patients who go past term are thought to have cephalopelvic disproportion, which can then be used as an excuse to do a cesarean section.

I remember very vividly, while serving my preceptorship in a small town, seeing a patient sent into the hospital by her general practitioner for a cesarean section to be done by one of the surgeons there, because the gestation had reached 44 weeks. The patient was duly admitted. In taking her history I found that the doctor had miscalculated her dates and that actually the patient's pregnancy was only of 40 weeks duration. The surgeon was so informed, the operation was rescheduled for the following week. Fortunately the patient, being a grand multipara, precipitated on the way to the hospital during the ensuing week. Cesarean section then, I think has a place in the treatment of post-maturity, but I also think that we, as obstetricians, have to be extremely careful that we are not pushed into unnecessary cesarean sections by the mother and, I hope not, by the pediatrician.

Dr. Hoeck:

Dr. Clifford relieved my mind on one point that had bothered me ever since I had seen a 3 lb. 12 oz. infant with a full term gestational period who

had physical findings suggesting postmaturity, including a thick leathery skin. This is possibly an example similar to the infant of premature weight that Dr. Clifford mentioned but with postmature characteristics, and shows that this sort of thing can happen.

The problem of restricting oxygen to prevent retrolental fibroplasia which Dr. Gordon has mentioned has confronted us also. There is relatively little oxygen being used now in the premature nursery, as compared with 6 to 7 years ago the rule being that it is used as needed. "As needed" means that most small prematures get some oxygen when they come into the nursery. We do not have a 24 hour rule, but I am sure that would work very well. Going through our records of oxygen administration to infants who weighed less than 3 lbs. at birth, I found that most infants got oxygen at least for part of the first day and several of them for as long as 4 days. In May 1953 we discontinued the use of high concentrations of oxygen as the result of a two-year comparative study in which 12 cases of retrolental fibroplasia appeared in a group of infants receiving high concentrations of oxygen for a prolonged period and only one case appeared in the group given oxygen for a limited time in concentrations less than 40 per cent. It requires constant emphasis and indoctrination of new nursing and medical personnel entering upon the scene for an appreciation of the proper use of oxygen; to use enough to correct cyanosis, but to constantly turn back the flow of oxygen to the minimum amount needed to prevent cyanosis and maintain a good color. Intermittent administration has been found useful and I do not believe harmful, as for example after feedings, when weak infants are prone to develop cyanosis.

If I can digress from this subject for a minute, there is a situation which has concerned me for a long time. Probably it started when Dr. Lewis Sweet was testing penicillin versus silver nitrate for eye prophylaxis in the newborn. From 1948 to 1950, out of approximately 4 thousand babies in each group (he was giving 50,000 units of penicillin intramuscularly to one, and silver nitrate locally in the conjunctival sac to the other) there were 6 cases of gonorrheal ophthalmia in the silver nitrate group and 7 cases in the penicillin group. He then started another study using 150,000 units of aqueous procaine penicillin intramuscularly as compared with silver nitrate in the conjunctival sac and found 3 cases of gonorrheal ophthalmia in about 4,000 babies in the penicillin group, and 10 cases in the same size group receiving silver nitrate. This may have been significant but was not enough to make any changes, so prophylaxis has been carried on up to the present time with silver nitrate. I have been trying to arrange a study in which we use no eye prophylaxis in order to find out what the incidence of gonorrheal ophthalmia actually would be, but I find that the laws prevent us from making studies of this sort. We have had our regulations revised for the

isolation periods of some of the communicable diseases, such as scarlet fever, measles, mumps, and polio, and intensive study of these diseases continues. In regard to ophthalmia neonatorum however, the law states there has to be silver nitrate or its equivalent put in the eyes immediately after birth. This puts us in the position of being compelled to use eye prophylaxis on the four million infants born yearly in the United States, but by the very nature of the law we are prevented from finding out just how much newborn ophthalmia is being prevented. I mention this to invite comment on any progress in this direction.

Dr. Burdick:

In the time that is left I am sure the panel members would be glad to answer questions or moderate any discussion which may be raised. Perhaps Dr. Clifford would like to say something before we start this general discussion.

Dr. Clifford:

I would like to mention what we have been doing about early use of oxygen in smaller premature infants. We have decided to use oxygen on a definite need basis. Of course, it does not take much push for us to decide whether a baby might need it, so most prematures who weigh less than 1500 gms. receive oxygen early. We have had the same problem in our community with the pendulum swinging in the wrong direction, so that frequently babies that most need oxygen are not given it. Many times we have to tell the family that we are taking a calculated risk, and they have to take their chances on a 5 per cent incidence of retrolental in a 2½ pound premature who is as black as your hat out of oxygen for a short period. Usually there is no question, and the family takes the risk of the eye disease. Thus far we have been lucky, but certainly many babies need their oxygen. I do not like to follow anything as a set rule; I hate prophylactic instructions, whether for oxygen or antibiotics, and I believe that every case should be decided on its own merit. This certainly has not affected our mortality when we have carefully watched the patient and treated as indicated.

As regards hyperbilirubinemia, I know that Dr. Diamond was responsible to a certain degree for this magic number, and I also know that when you talk to him about a case where the bilirubin is around 20 mg. per cent he will say, "20 is not too solid a figure; actually, 25 is a pretty good figure." If there is no evidence of hemolysis, if the spleen is not palpable, if the rest of the picture looks good, if the baby's bilirubin level is rising due to some dehydration, we sit tight. I think our big study for next year is going to be the accurate evaluation of the bilirubinemia in true, hemolytic disease of

the rH type, as compared to that due to ABO incompatibilities, and that due to hemolysis associated with sepsis. It may be that we are dealing with different hemoglobins, as Dr. Diamond pointed out in his discussion at BuckHill Falls, or perhaps there are different thresholds for bilirubin. All of us do not even use the same colorimetric index for bilirubin, so the bilirubin level in Boston may be entirely different from a bilirubin level in another community where a commercial bilirubin is the standard. For example we had a baby who was a candidate for an exchange transfusion, but I did not think his general condition could stand it. After all, some of these prematures are also suffering from hyaline membrane disease. We sent the same bilirubin sample to two other laboratories and got different results from each, so we took the lowest one and stuck by our guns. I do not think that we should be satisfied with only one reading. I think we should construct a curve, because it makes a difference whether it is 25 mg. of indirect bilirubin per 100 ml. on the fifth day or on the second day. It is also essential to observe the pitch of that curve and what is happening to the red count.

I would like to say that I did not mean that cephalopelvic disproportion *per se* was the etiology of post-maturity in a great many women. As a matter of fact, it is not, because, as Dr. Barter pointed out, the x-ray pelvimetry studies that most clinics are now doing show that a true cephalopelvic disproportion is a relatively infrequent situation. All the studies from these obstetric centers point out that the main difficulty in the women who are postmature has been the increased incidence of occiput posterior and transverse presentations.

This question of silver nitrate and the gonococcal ophthalmia is a very interesting one. The old literature on the subject states that silver nitrate has reduced the incidence of gonococcal ophthalmia 70 per cent, or some such figure as that. Crede's original article would never be accepted for publication today, and there has never been any follow-up on it. However, he never read into his figures the interpretation and prophylactic value that our laws have given his results, and even in those days the incidence of gonococcal ophthalmia would have been phenomenal if the incidence as given in his little series had been correct. We know that silver nitrate is not a true prophylactic agent. For example, we have a baby in our nursery right now who has had adequate silver nitrate prophylaxis, and yet had gonococcal ophthalmia by smear; we also recovered gonococci from the mother's cervix. I do not know how to get a gonococcal ophthalmia control series, although I do think that the ideal way to handle the problem today is to have constant observation and only treat the baby when he shows signs of the disease. The trouble, of course, with the prophylactic intramuscular penicillin treatment is that it may mask lues which will do more harm than the gonococcal ophthalmia. There is an interesting study going on in our

hospital now on inclusion blenorrhea. Recently we had had about seven babies who had a conjunctival discharge that we thought was a silver nitrate reaction which on study turned out to be inclusion blenorrhea. We have been able to recover virus as well as to identify the inclusion.

Dr. Burdick:

The panel members will now answer any questions from the floor.

Question:

Has there been a long enough period of follow-up on these postmature babies (stage II & III) to determine whether there has been any evidence of brain damage and so on?

Dr. Clifford:

There has not been a long enough follow-up period on them to give you any figures on it, but we have had some who have been permanently damaged. We had one baby in our original series whose mother thought the baby had a convulsion *in utero* because of the sudden tremendous kicking and activity. The baby was about three weeks postmature when delivered, and exhibited the picture of extreme malnutrition and peeling together with a great deal of respiratory difficulty. I transferred the patient to Children's Hospital because the child was having convulsions, and the lumbar puncture showed bloody fluid containing a large amount of protein. At 2 weeks of age, although the head circumference was entirely normal, I injected 10 cc. of air into the ventricular system, and by x-ray was able to show that there was already tremendous atrophy and destruction of the cerebral cortex, and an increased intraventricular space. At 6 years of age, this baby's head size was about one centimeter bigger than on the day it was born, and its cortex was as thin as a piece of paper. Another interesting patient whom we saw had developed epilepsy, so we went back to the original records and found that she had had convulsions at birth, was postmature, and we commented at that time on both the extreme staining of the vernix and her malnutrition. This child now had the encephalographic changes typical of petit mal epilepsy, and whether this is related to these early conditions or not, I do not know.

Walker feels very strongly that if he does not allow these patients with yellow amniotic fluid to go through labor and have further injury in addition to the anoxia already present, he will reduce the morbidity and central nervous system damage which we are now seeing as cerebral palsy and spasticity of various types. I think that this whole approach toward an attempt to prevent added damage may benefit our children not only from the point of view of mortality but from the point of view of mor-

bidity as well. I am reminded of Dr. Eastman who feels that with the dramatic reduction in neonatal mortality it is now our duty to concentrate on perinatal morbidity. He thinks, among other things, that the flooding of a mother with oxygen during the final stages of labor should be almost a must in every clinic in the hope that it will help prevent some anoxic injury which later might cause morbidity. The science of obstetrics has advanced so that the mortality is extremely low, and as we become more mature, we are now trying not only to get live babies, but to get them with the minimum of injury.

Question:

Comment on the benefit or non-benefit of mist or high humidity for premature babies delivered by cesarean section.

Dr. Clifford:

I think that the advantage of using mist for the treatment of babies after cesarean section has been pretty well disproven. The only advantage of a high humidity atmosphere in the form of mist is that it cuts down the insensible water loss during the long period of starvation while the babies are in extreme respiratory distress. Silverman and Anderson demonstrated in their first study in comparing mist vs. Alevaire®, there was no advantage one way or the other. The next study was between mist and no mist in treatment of hyaline membrane disease, and there was no difference in results between these two groups of patients. In addition, there is a distinct danger from the fog or the mist and that is, as Dr. Gordon has seen also, the macerations of the skin and pseudomonas infection; therefore I do not use mist routinely anymore even in tiny prematures.

Silverman in a paper to be published shortly in Pediatrics has found that babies treated in 90–100 per cent high humidity (not mist) have a statistically significant lower mortality and morbidity rate than those kept at 30–40 per cent relative humidity.

Question:

Comment on the use of polyethylene gavage feedings in prematures that weigh less than 1500 grams.

Dr. Clifford:

Although others are enthusiastic about it, we do not use it at all because our nurses are being trained in gavage feedings, and we think it is useful to move a premature baby and handle it. Also we have seen secondary infection around the tube even though it is changed every two days, and there have been some reports of gastric ulcerations near the tip of the

tube. However, I think you can not be criticized for using it, and it is a very useful thing in certain clinics where there is not adequate nursing power to pass a tube at each feeding time. The surgeons are also getting away from these tubes. I heard Dr. Gross talk with Dr. McLendon in Miami and he is very much opposed to the continued use of the oral polyethylene tube; in the prematures on whom he does gastro-intestinal surgery, he prefers to sew a tube right into the stomach and he says this is becoming the way to handle these problems.

ANNOUNCEMENT

The Annual Picnic sponsored by the Clinical Proceedings of Children's Hospital was held May 30, 1957 at Hollow Tree Farm. A beautiful day brought out a large attendance. The Medical Staff and Resident Staff fought for supremacy on the soft ball diamond, and the coveted trophy was carried off in triumph by the oldtimers.

Grateful appreciation is expressed for the generosity of the Medical Staff, Dr. Homer King Vann, and Mead Johnson and Company who made this enjoyable sporting event possible.

DIAGNOSTIC BRIEF

DIAGNOSTIC X-RAY BRIEF

ENLARGED PARIETAL FORAMINA

Isidore Lattman, M.D.*
Joseph M. LoPresti, M.D.†

This rare congenital anomaly has been known for the past 300 years[1, 2, 3], and is important because it may lead to serious errors in diagnosis. The parietal bone may exhibit near its upper posterior angle a minute opening through which an emissary vein passes. This opening is called the parietal foramen and its size does not usually exceed a millimeter in diameter. At times, this very small parietal foramen is bilateral. On rare occasions it is large enough to admit a finger[4]. Large parietal foramina apparently result from an irregularity in the process of ossification.

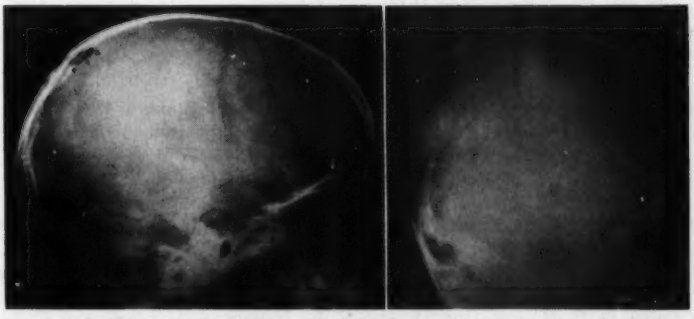

FIG. 1. AP and lateral views of the skull demonstrating bilateral parietal foramina (arrows). Bilateral bony defects with smooth margins are seen in the upper posterior angles of the parietal bones.

Few, if any, symptoms are caused by the anomaly and as a rule the patients are in no way inconvenienced unless pressure or tension is put upon the scalp overlying the foramen. These foramina have never been reported to be the site of meningoceles. Occasionally, when very large parietal foramina are present, one may palpate a defect in the calvarium through the scalp.

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Roentgenograms of the skull are typical, and most frequently, this anomaly is diagnosed coincidentally when the skull is x-rayed for some other reason. Bilateral circular bony defects with smooth margins are seen in the upper posterior angles of the parietal bones (Figure 1). The edges of the defects are bevelled so that their size is smaller in the inner than in the outer table of the skull. These defects, therefore, resemble craniotomy burr holes.

The importance of this congenital anomaly is in the fact that when it is found, it may be misinterpreted. The roentgenologist should be familiar with its appearance and occurrence. Enlarged parietal foramina may be confused with epidermoidomas (cholesteatomas), reticuloendothelioses, metastatic neoplasms, trephine openings, cleidocranial dysostosis, syphilis, meningoceles, and osteoporosis circumscripta. A knowledge of the anomaly is all that is needed for differentiation.

Of ancillary interest is the hereditary nature of enlarged parietal foramina. Goldsmith[5] examined 56 members of the Catlin family distributed in five generations; 16 exhibited the anomaly, 13 in members of male sex and 3 female. He called the anomaly "the Catlin mark". More recently, Pepper and Pendergast[6] in three generations of another family found enlarged parietal foramina in 8 of the 10 members examined.

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